Report of the Checking Committee on the TRIUMF 1995 Data Analysis

Checking Committee

June 30th, 1997

1 Introduction

This note describes the report of the Checking Committee on the TRIUMF 1995 Data analysis of $K^+ \to \pi^+\nu\bar{\nu}$. The members of the Checking Committee are L. Littenberg (BNL), R. Strand (BNL), C. Witzig (BNL), D. Marlow (Princeton), T. Numao (TRIUMF), T. Nakano (Osaka) and Y. Kuno (KEK).[1] The goal of the committee is to examine the TRIUMF 1995 data analysis of $K^+ \to \pi^+\nu\bar{\nu}$ by the end of June, 1997. However, the acceptance calculation is not included in its examination.

2 Activities

The activity of the checking committee can be separated into two; namely

- Examination of the TRIUMF background studies, and
- Examination of the "event".

Regarding the TRIUMF background studies, it was recognized in the committee from the very beginning that the estimation of charge exchange (CEX) background has to be redone more carefully. Active discussions on CEX has been made in the committee.[2]

Since manpower in the committee and time allocated are limited, the committee decided not to do their own independent background analysis, but rather to make a list of the items to be checked and asked the TRIUMF analysis to answer them. This mechanism did not bring in independent investigation in a complete sense. However, in a given time, it was thought to be the best way to reach conclusion in a timely manner before the deadline. The activity in the subsequent analysis group, which is carrying out an independent approach, would give complementarity in the examination of the TRIUMF background studies.

[1] R. McPherson was asked to join, but he never participated in the meetings.

[2] New data for CEX were taken and a new note on CEX has been written and distributed as a result.

3 Examination of Background Studies

The committee made a list of the questions to be studied by the TRIUMF analysis group. The questions can be grouped into two categories;

- possible correlations in the background estimations, and
- un-estimated (or new type of) backgrounds.

In addition to these, it was pointed out that the **systematic errors** in the background estimation have to be estimated.

The questions on the **correlations** in the background estimations are as follows;

1. *check correlation between PV and PISCAT cuts.*
2. *check for possible triple correlations such as among E/P/R*
3. *check possible correlation of RSDEDX with E/P/R*
4. *check an effect of the R-E box cut which is fixed for the* $K_{\mu 2}$ *kinematic function but not for the* $K_{\pi 2}$ *function.*
5. *check the two beam background with short spatial separation in either BWC and B4.*
6. *check possible correlation caused by the requirement of* $K_{\mu 2}$ *stopping within layer 20.*

The questions on the **un-estimated backgrounds** are as follows;

1. *check charge exchange (CEX) background for* $T_K > 50$ *MeV.*
2. *check single beam background with high dE/dx in B4.*
3. *check double beam background with the one* K^+ *charge exchanged in either degrader or cherenkov counter or further upstream in the beam.*

All of the questions were reasonably answered. The answers from the TRIUMF analyzers are shown in the Appendix A of this note for further information.

4 Examination of the "Event"

The activities in the event examination can grouped into two;

- scanning of mixed samples in which the "event" is included, and
- investigation of the "event".

For the former, the TRIUMF analyzers provided three different samples. Some members of the Checking Committee scanned them to identify the "event". For the latter, some members looked at the "event" at either ntuple or raw data. And also we examined it based on the TRIUMF memo 6 which is written about the "event". Since the "event" is quite clean, the Committee did not find any serious problems, although some questions shown below were raised but none of them were found to be fatal to kill the "event".

1. *pion/muon pulses in TD, relation between their timing and pulse height*

2. *left/right ambiguity of the hits in the first layer of UTC.*

3. *Early hits at the incident K coming time.*

The first one is described in Appendix B. The second and third ones are already described in the TRIUMF memo 6.

5 Conclusions

After our investigation, the conclusion from the checking committee is as follows;

- `The checking committee has examined the background estimations in the TRIUMF analysis of the 1995 data, and found no serious loopholes in their estimations.`[3]

- `The checking committee has examined the event, and found no serious problems for that event as a` $K^+ \to \pi^+ \nu\bar{\nu}$ `candidate.`

[3]In fact, at the time of writing this note, we have NOT discussed among the whole members the final report on CEX that was distributed June 30th.

Appendix

Background Studies

The questions were answered by the TRIUMF analyzers. In the following, their answers are presented for information.

Correlations

Correlation between PV and PISCAT cuts

This is explained in detail in the first memo to the committee.

A.1.2 Possible triple correlations such as among E/P/R

First of all, just to be clear, a triple correlation between E, P and R does not affect the background estimate. The $K_{\pi 2}$ background is estimated by applying the final E, P and R cuts so any correlation between the three variables is already built into the background estimate. However, a triple correlation can affect the function inside the box.

Before we get into this further, let's get some idea of what level of correlations exist between E, P and R. We do this by comparing the rejections of the energy box cut, momentum box cut, and range box cut under different conditions. For example, we can measure the P box rejection with and without applying the E and R box cuts.

The following table shows the results for the $K_{\pi 2}$ kinematic background sample after all the kinematic cuts have been applied except the E, P and R box cuts.

	N(peak)	N(tail)	R
range	60055	206	292 ± 20
energy	58255	1326	44 ± 1
momentum	57389	1841	31 ± 1

Next we have the results after applying just the E box cut:

	N(peak)	N(tail)	N(tail expected)
range	1322	3	4.7
momentum	1250	60	41.9

where N(tail expected) gives the expected number of events if the rejections measured in the first table were applied (i.e. assuming no correlation).

Similarly after applying just the P box cut:

	N(peak)	N(tail)	N(tail expected)
range	1811	29	6.6
energy	1773	60	42.5

From this we get some idea of the level of correlation:

R vs E:	not clear statistically
R vs P:	$29/6.6 = 4.4 \pm 0.8$
E vs P:	$60/42.5 = 1.4 \pm 0.2$

From this we conclude that the correlation between range and momentum is the biggest effect for the $K_{\pi 2}$ peak. This is not surprising, since target range is used to compute the momentum and the dip angle measurement from the UTC is used for both range and momentum. Energy and momentum are mildly correlated; one known mechanism is through the dip angle as described in the memo 3 and in the first note to the checking committee (memo 5). The correlation between range and energy appears to be small; the two are known to be correlated through the last counter energy, but the last counter range is not that sensitive to small changes in the last counter energy. The application of the dE/dx cuts can also correlate range and energy; this effect is included in the above measurement where the dE/dx cuts have been applied.

Now, getting back to the original question, when we constructed the function inside the box, we had to do something to increase the statistics (otherwise, we would have to form the function with just the two remaining events). Since the largest correlation was between R and P, we wanted to be sure to build that into our function; we therefore increased our statistics by not applying the E box cut. Could this change the shape of the function in a dramatic way? Probably not. Energy and range are not highly correlated, as we saw. Energy and momentum are correlated through the dip-angle effect, but as mentioned in the first note to the checking committee, we removed this correlation by correcting both energy and momentum for their dip-angle dependence.

A.1.3 Possible correlation of RSDEDX with E/P/R

The RSDEDX cut was applied before the R, E, P box rejection was measured and the range-energy, range-momentum and energy-momentum correlations were examined. Thus any additional range-energy correlation introduced by the RSDEDX cut is accounted for.

A.1.4 Effect of the R-E box cut which is fixed for the $K_{\mu 2}$ kinematic function but not for the $K_{\pi 2}$ function

The $K_{\mu 2}$ kinematic function outside the box was determined with fixed cuts in range, energy and momentum (except for the high-side momentum cut). However, when the cuts for all the backgrounds are loosened, the low side of the range, energy and momentum box are loosened (for the $K_{\pi 2}$ background); this can change the ratio of muon band and $K_{\mu 2}$ events from the ratio that was used when the $K_{\mu 2}$ kinematic function was determined.

The range versus momentum for the sample used to determine the $K_{\mu 2}$ kinematic function outside the box is shown in figure 1 (top); energy versus momentum is shown in figure 1 (bottom). It is safe to say that loosening the low side of the energy and range boxes does not introduce more muon band events since the loosest momentum cut is at 209 MeV/c. However, loosening the momentum cut from the nominal box position of 211 MeV/c to 209 MeV/c can have an effect. To deal with this, we measured the $K_{\mu 2}$ kinematic function with the momentum cut fixed at 209 MeV/c as was described in memo4. This introduces slightly more muon band events than one would get with a sliding momentum cut, but the difference is small since the muon band distribution is falling.

A.1.5 Two-beam backgrounds with short spatial separation in either BWC and B4.

The two-particle beam background assumed that the degrader mixed things up enough so that tagging two tracks in B4 did not bias the spatial separation of the two tracks in the BWPC's. To check this, a simple toy Monte Carlo was written.

The formula from the Particle Data Book giving the rms of the spatial displacement of a track undergoing multiple scattering was used with the following inputs:

length of degrader:	53.5 cm (assumed to be entirely BeO)
radiation length of BeO:	31.7 cm
K^+ momentum:	800 MeV/c

giving an rms of 0.83cm. This is probably an underestimate since the kaon is losing momentum as it traverses the degrader (although at the same time the lever arm is getting shorter).

The toy Monte Carlo generated events with two tracks (uniformly distributed), scattered them with a Gaussian with sigma=0.83cm, and checked their spatial separation at the B4 counter. Two tracks were considered to be overlapped in the BWPC when their separation was less than 2.54mm (the wire spacing is 1.27 mm and the wires are read out in pairs). (This is from old NIM paper; is it still true???) For the B4 fingers, the widths were taken to be 1.2cm, 1.8cm and 2.1cm.

100,000 events were generated.
6257 events had a single hit in the BWPC (or 6.3% of 100,000).

72,663 events had 2 hits in B4 (1.2cm fingers)
3916 of these had a single hit in the BWPC (5.0%)

67,613 events had 2 hits in 1.8cm fingers
3068 had single BWPC hit (4.5%)

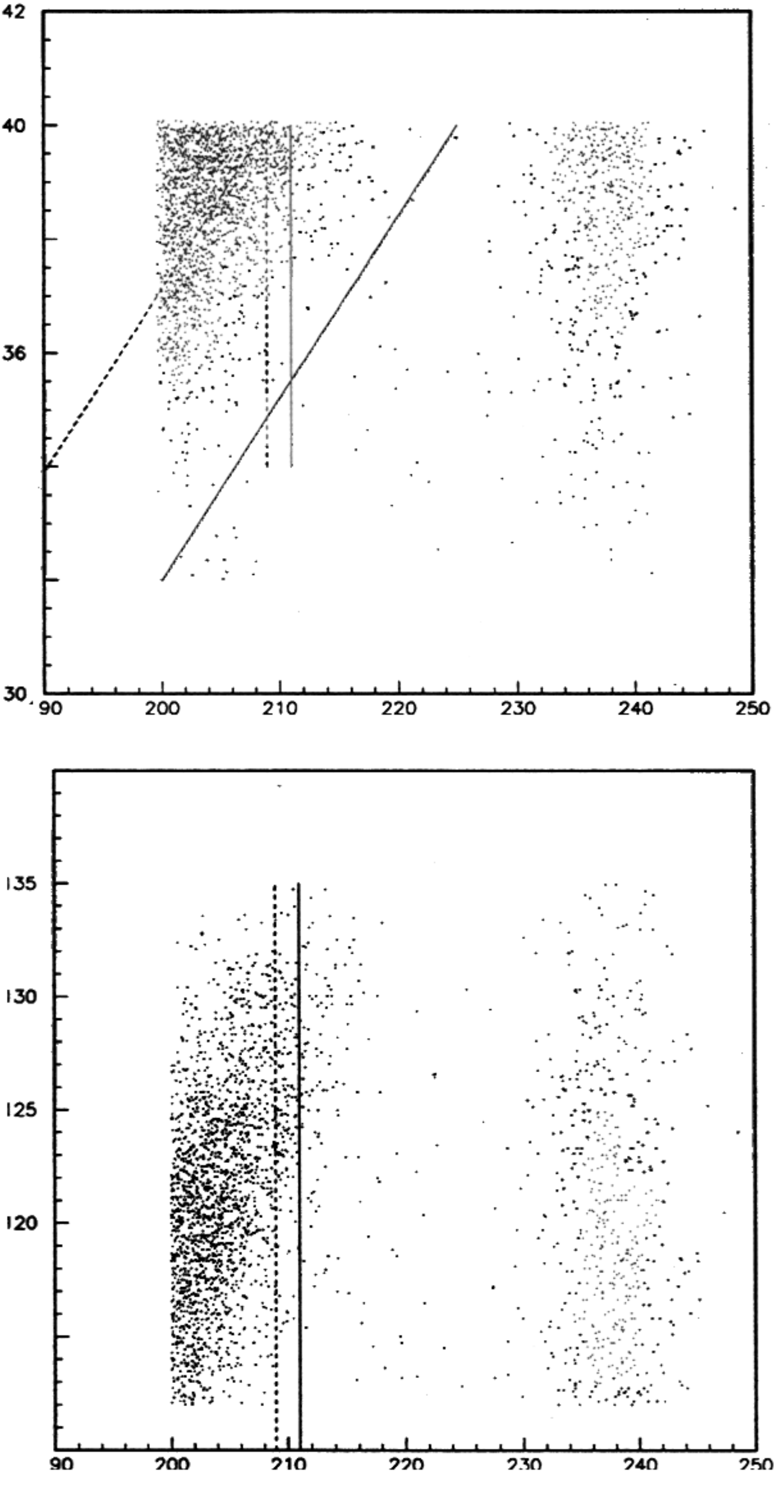


Figure 1: top is range momentum; bottom is energy momentum.

62,419 events had 2 hits in 2.1cm fingers
2603 had single BWPC hit (4.2%)

So, indeed, by selecting events with 2 hits in B4, one gets slightly fewer events where both tracks overlap in the BWPC but the deficit is not huge. The factor ranges from about 1.2 (6.3%/5.0%) for the 1.2cm fingers to 1.5 (6.3/4.2) for the 2.1cm fingers. As mentioned above, the true factor should be less than this since the scattering through the degrader is larger because the kaon is slowing down as it goes through the degrader.

A.1.6 Possible correlation caused by the requirement of $K_{\mu2}$ stopping within layers 20.

We assume this reads "what is the bias in the acceptance study due to the selection of $K_{\mu2}$ events with layer 21 veto?". By making layer 21 veto, we select $K_{\mu2}$'s with large dip angle. However, we don't have apparent acceptance loss which has a dependence on dip-angle. Obviously, F_s measurement depends on this layer 21 cut, and a detailed monte Carlo study is under way including a check on layer 21 cut dependence.

Un-estimated backgrounds

Charge exchange background.

An additional study of the charge exchange background will be described in the next note to the committee. What we did is to take out all the CEX related cuts;

- delayed coincidence for T_K <50,
- delayed coincidence for normal events at 1.5nsec instead of 2nsec,
- no target gap cut (dist_targf)
- no maximum pion fiber energy cut (epi_tg,epimaxk)
- no kink cut ((drpmax-drpmin)/rtg)
- no backscatt cut (phivtx)
- no target photon veto (tgpvcut1)

After scanning all skim6 data, 15 events are left, one of which is of course the event. They contain other backgrounds, such as 2 beam background and K decay in flights. After hand-scanning them, 4 events are left as possible CEX candidates (cex_tg.ps). The first one is very convincing and has a proper K_L mass (501MeV/c^2). After estimating the extra rejection by above set of cuts, we can get a more reliable estimation of the CEX background. Another approach we explain in the next memo is to estimate how likely the event be as a charge exchange background.[4]

[4]The new note on CEX is available after this message.

A.2.2 Single beam background with high dE/dx in B4.

This represents the K decays-in-flight background. As described in the memo 3, decays-in-flight background is properly taken into account in the single beam background study as long as the delayed coincidence rejection is the same for decays-in-flight and single pion scattering. In the memo 3, a possible source of correlation due to target Z dependence, which is the only model we came up with, is dealt with. We also tried to isolate decays-in-flight independently, but we could not clean-up the contamination from ordinary K^+ decays.

A.2.3 Double beams with the one charge exchanged either in degrader, or cherenkov counter or further upstream in the beam.

As long as the background has a signature of 2 hits in BWPC, which is the case here, it is included properly in the 2 beam background study. To iterate, the entire cuts except for BWPC 2 track cut is applied on the BWPC tagged sample, which has 2 tracks in BWPC. This sample includes this double beam with one charge exchange case since the degrader is downstream of the BWPC's. The rejection of BWPC 2 track cut is measured by a sample with 2 hits in B4, whose rejection should also be the same for 2 beam charge exchange case with two K^+ passing through BWPC's. Dividing the number of remaining events in the BWPC tagged sample with BWPC rejection, we got the expected number of double beam backgrounds events.

As pointed out by Peter Meyers before, the double beams with one particle interacting in the Cherenkov radiator is not taken into account here. However, this contribution is estimated to be 10% of double beams with CEX in the degrader by the discussion as follows: The mean interaction cross section is at most a factor of 2 larger in the Cherenkov radiator than in the degrader. The amount of the material is 2 grams in the Cherenkov and 150 grams for the degrader. The solid angle to look at the kaon blob from the interaction point is 1/10 from the Cherenkov compared to that from the degrader. If the interaction happens in the Cherenkov, we lose the BWPC 2 track rejection of 36. Thus,

$$\frac{N_{\text{cerenkov}}}{N_{\text{degrader}}} = 2 \times \frac{2}{150} \times \frac{1}{10} \times 36 = 0.1, \tag{1}$$

where N_{cerenkov} and N_{degrader} are a number of the background contributions at the Cherenkov counter and at the degrader, respectively.

Yoshi raised a possibility of accidental K_L produced in other places upstream of the BWPC1. The largest contribution would be in the collimator right after the last bending magnet. Only a few % (say 5%) of the beam is expected to hit the collimator. The relative solid angle to see the K^+ blob from the collimator compared to that from the degrader is smaller than 1/50 due to the long lever arm. The interaction probability can go up at most a factor of 2 compared to the degrader where more than half of K^+ interacts. Thus, the collimator contribution, $N_{\text{collimater}}$, relative to the degrader contribution, N_{degrader}, is smaller than

$$\frac{N_{\text{collimator}}}{N_{\text{degrader}}} \quad 0.05 \quad \mathbf{2} \qquad 36 = 0.07. \tag{2}$$

B Event Examination

B.1 π/μ timing and pulse heights

Laurie found a slight inconsistency on the pion and muon positions derived from the pulse heights and timing in the pion stopping layer, Layer 14. The following is his question.

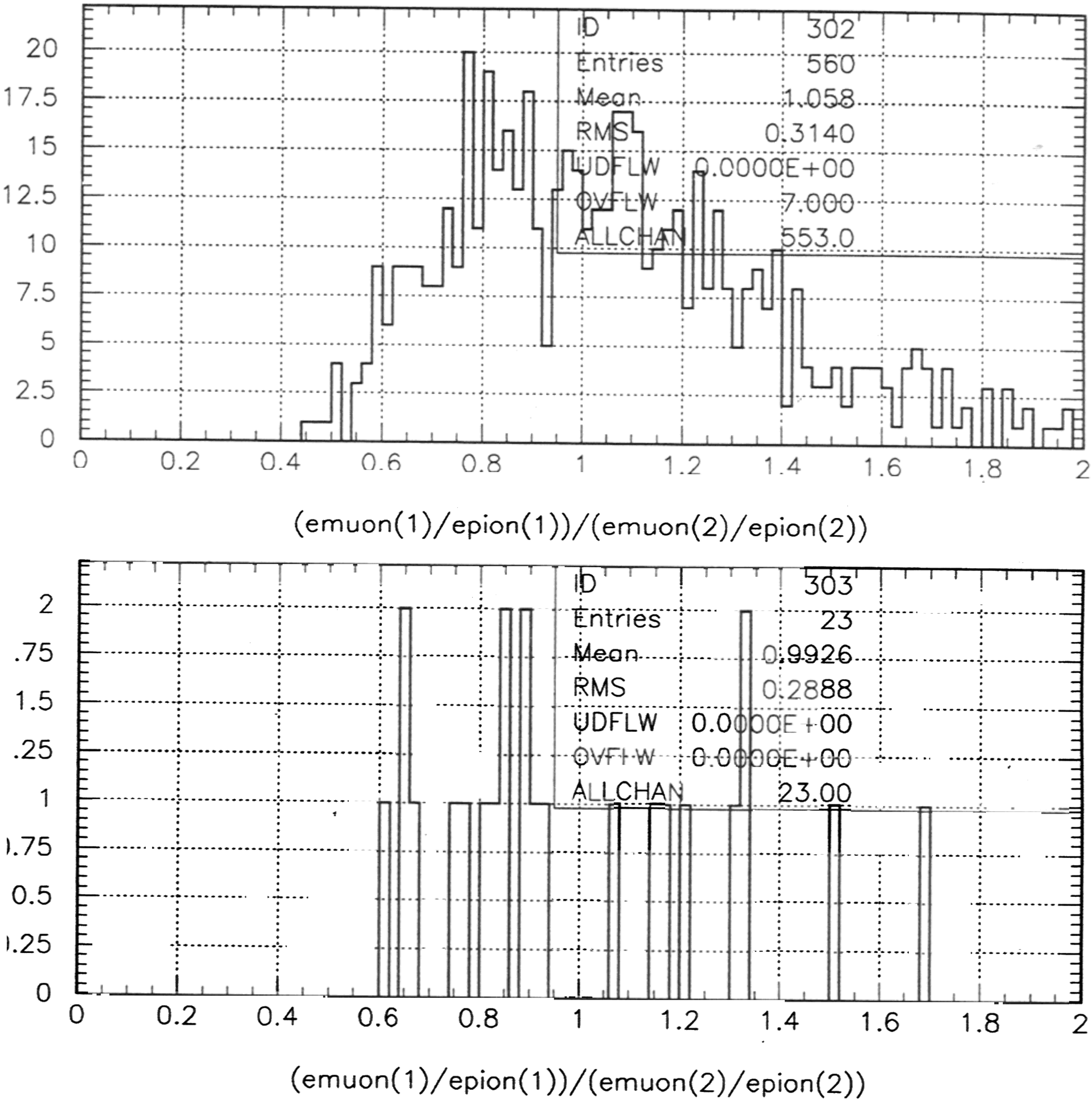
"From Fig.4 in the memo 6, both the relative π and μ timing and pulse height (PH) agree that the μ was further downstream than was the π. Very roughly speaking the timing suggests that this difference is 11 $\pm$ 6 cm (I assume effective propagation time of $c/2$), and the PH suggests this difference is 33 $\pm$ 13 cm (I'm assuming one sigma is 13). The fact that both these numbers are positive should count against the event. Now I don't believe this implies anything is wrong with the background studies, but it is not confidence-building. A couple of things I'd like to know from Akira are (1) what does Fig 40 in Memo 6 look like if confined to events with about the same pi-mu time difference and similar pion PH to the event ? and (2) what does Fig 40 look like if confined to stops in the same counter as the event ?"

To respond this, the plots were made by the TRIUMF analyzers in figure 2. The top figure corresponds to (1) a width Δt = -1.35 $\pm$ 0.15 nsec. The bottom figure corresponds to (2), where I could not get much statistics even using the entire tagged π-scattering sample. From this, it is concluded that it looks like neither selection is grossly different from the overall sample.

C History

The following is a brief history of the committee activities.

April 20th	The checkers are formed.
April 28th	Memo 5 (about the events around the box) ready.
May 7th	1st meeting
	send a list of the checking items of background studies
May 14th	**2nd meeting**
May 16th	Answers from the TRIUMF analysis
May 23rd	Memo 6 (about the "event") ready.
June 10th	**3rd meeting**
June 23rd	**4th meeting**
June 26th	Memo 7 (about CEX) ready
June 27th	**5th meeting**
June 30th	**6th meeting**
July 1st	E787 analysis meeting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Entries 560
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RMS 0.3140
UDFLW 0.0000E+00
OVFLW 7.000
ALLCHAN 553.0
20
17.5
15
12.5
10
7.5
5
2.5
0
0 0.2 0.4 0.6 0.8 1 1.2 1.4 1.6 1.8 2
(emuon(1)/epion(1))/(emuon(2)/epion(2))
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Mean 0.9926
RMS 0.2888
UDFLW 0.0000E+00
OVFLW 0.0000E+00
ALLCHAN 23.00
2
.75
1.5
.25
1
).75
0.5
).25
0
0 0.2 0.4 0.6 0.8 1 1.2 1.4 1.6 1.8 2
(emuon(1)/epion(1))/(emuon(2)/epion(2))

Figure 2: